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Discussions

Interactive comment on "Decadal-scale relationship between measurements of aerosols, land-use change, and fire over Southeast Asia" *by* J. B. Cohen and E. Lecoeur

Anonymous Referee #1

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This paper uses meteorology and land use data over Southeast Asia to build a statistical model to predict AOD. The discussion focuses largely on the extent to which different variables (NDVI, LAI, fire counts, precipitation) contribute to the observed AOD patterns. The focus is on building a predictor for the MODIS AOD, but the results of the model are also compared to AERONET and MISR observations.

This was a difficult paper to review. To be honest, while I think I understand most of what was done in this study, I am still not certain why it was done. The authors also don't provide any particular context for this analysis, or what applications of this are. If the purpose is to model the AOD in this region then it isn't clear why this method is

preferable to just using the remote sensing data directly. If it is to understand how AOD changes as land cover/climate changes in this region, then again it seems like this is a question best answered by regional/global models in concert with observations. The main results (that fire counts and changes in surface cover are a good way to tell when burning is happening) are well-known and indeed provide the basis of many biomass-burning emissions inventories. The Conclusions to the paper mostly read like a description of the variability of biomass burning AOD in this region, which is fairly well-known from other model/observational studies, and the issues highlighted about sampling difficulties in this region are also similarly well-known (and much of this prior literature is not cited). Unless the authors are able to provide a clear and convincing explanation of the utility of this study to answer new scientific questions, then I would unfortunately recommend that the paper is rejected on the grounds of lack of scientific significance, particularly for a fairly high-profile journal like ACP. In addition to this, the paper is quite similar to a prior study by one of the authors (EOF analysis using MISR, AERONET, TRMM), Cohen (2014), so this seems only incremental on that in terms of trying to understand the biomass burning system.

There are also quite a few technical errors in the text (particularly related to the satellite data and definitions about aerosols, indicated below) and some shortcomings of the analysis and discussion (for example there is very little discussion of observational error in any of the data sets used), as well as some parts of the analysis and interpretation which remain unclear. These issues would also require some major rewriting of the manuscript to address, in addition to the aforementioned concerns about scientific novelty/significance were addressed.

Page 26898, lines 17-20: This is not the definition of AOD. The word 'proportion' implies that it is a fraction, which is incorrect. AOD is the vertical integral of atmospheric extinction (scattering and absorption) due to aerosol particles. This is quite different from the proportion of light which doesn't reach the Earth's surface, which is the authors' claimed definition. Much of the light scattered by aerosols reaches the Earth's surface.

Page 26898, lines 20-22: This is incorrect. AOD cannot be directly measured by any of these instruments. They all measure reflected/transmitted solar radiation and then, with some assumptions, retrieve AOD. The assumptions required for AERONET result in substantially lower uncertainty than those for satellite instruments, but the AERONET data (even direct Sun observations) are still very much a retrieval and not a 'direct measurement' as the authors assert. To state otherwise is very misleading.

Page 26898, line 27: I don't think that the phrase 'one to one' is really appropriate here. It has mathematical implications which are simply not true (that emissions map directly to AOD, which ignores all other aspects of the aerosol life cycle).

Page 26899, line 16 (and elsewhere): what exactly is 'FireMask'? Is this a gridded fire count? Are there corrections applied to it? Or is it a shapefile indicating areas where fire(s) were detected? The term 'FireMask' sounds like a computational variable rather than a physical quantity.

Page 26900, line 13: Figure 3 is mentioned before Figures 1 or 2. Figures should be numbered in the order in which they are first mentioned in the text.

Page 26900, lines 21-22: MODIS Terra and MODIS Aqua are satellite instruments, not satellites (Terra and Aqua are the satellites, MODIS one of several instruments upon them). This mistake is made in the Abstract as well.

Page 26900, line 22: Which version of the MODIS aerosol products is being used? This is not mentioned. The reference given here is to an obsolete version of the MODIS products, and so is not appropriate, unless the authors are using this old version.

Page 26900, line 24: MODIS aerosol products are not provided at 0.1x0.1 degree resolution. The paper is incorrect. The spatial resolution of the MODIS aerosol products is 10x10 km2 at nadir (which is not the same as 0.1x0.1 degrees), but increases with scan angle due to the sensor scan geometry and the curvature of the Earth. The au-

thors should be more exact about what MODIS data are being used and how, if at all, this is quality filtered and aggregated. For example when going to 8 day resolution is it averaged on the spatial grid and then day by day, or are all points in the period averaged at once?

Page 26900, lines 24-26: Again, see the earlier comment about what AOD is.

Page 26900, line 26: It isn't really correct to say that AOD is a function of 'washout from precipitation'. Really the atmospheric aerosol loading is a function of washout of precipitation. AOD is a function of the atmospheric loading and the optical properties. So this sentence should be corrected.

Page 26901, line 1: AOD is also not a function of the vertical distribution of the aerosols, since AOD is by definition an integral over the vertical column. Of course the vertical distribution of aerosols may vary, and this affects the aerosols' radiative effects, but it is incorrect to say that AOD depends on the vertical variation of aerosol properties.

Page 26901, lines 9-10: It isn't clear what versions of LAI, NDVI, or FireMask data sets are used, or what quality filtering is applied, and again I am not convinced that the various references provided here are correct or relevant for these data sets.

Page 26901, line 26: Is top of atmosphere or top of canopy (i.e. with a basic atmospheric correction) NDVI used? It isn't clear because the authors don't say.

Page 26902, lines 12-13: Again what data version, and what's the reference?

Page 26902, lines 19-21: What temporal resolution is the AERONET data used at? All points? Sampled to the satellite overpass times? Daily averages? Monthly averages? This should be stated, and is potentially important, due to the high diurnal and day-to-day variability of AOD in this region.

Page 26902, line 25: Holben et al. (1998) is an AERONET reference, not a MISR reference. Again, what data version is used for MISR, and what's the reference? I also don't think that MISR adds much value to this analysis. Since it has a narrow swath width (around 360 km, so revisit time around 5-7 days at this latitude, I think), the number of potential AOD observations over a month is very low (4-6 overpasses compared to near-daily for MODIS). When you consider that half or more of the data are likely to be rejected due to cloudiness, there will be very few days contributing to this monthly average, so I would have strong doubts about how well MISR represents the monthly mean AOD, given the potential day-to-day variability in aerosol loading. MISR also has AOD retrieval uncertainties of a similar level to MODIS, so it isn't really a benchmark to evaluate the predictive model in the same way that AERONET might be. And since the model is built to MODIS, it may not even be meaningful to test it against other AOD data sources, due to the different sampling/uncertainties.

Section 2.2, general: no discussion of the uncertainties on any of these MODIS data products is discussed. This is particularly important in this region because AOD errors are a function of AOD (which is often high here), and AOD changes will also alias into apparent changes in LAI and NDVI due to limitations in the atmospheric correction for those surface products. Indeed, LAI and NDVI data are also somewhat related since they are both ways to get at the surface cover from a similar subset of MODIS measurements (although the processing algorithms are different). Again, the authors present the various data sets as though they are entirely independent measurements of entirely different things when this is simply not the case. Undetected thin cirrus clouds will also bias all these data sets. Thus there are some potentially significant and potentially correlated sources of error in these data sets, which are known to lead to problems in regions of high cloudiness such as this, which are completely ignored by the authors. The authors also don't, as far as I can see, directly state which years of data were used.

Section 2.2/2.3: It isn't clear to me why 8 days is an appropriate time period for this analysis, since AOD, which is the quantity which the authors wish to predict, has a much shorter lifetime than this. It also isn't clear whether the 8-day average is a reasonable time scale for wanting to predict AOD on, since the mean AOD is probably not a good

representation of the actual aerosol loading (due to its short lifetime it will often be much higher or lower than the mean would imply) or radiative effects (since these are not linear with AOD). NDVI and LAI are probably more stable over this period, but these aren't the variables which the model is aiming to predict.

Page 26906, lines 10-11: Some more explanation of what these percentiles mean, and how they were chosen, is necessary.

Page 26906, lines 14-16: This 5% threshold is arbitrary. The authors provide no evidence to suggest that real changes will cause more than a 5% contribution to the total variance, or that retrieval/sampling errors will contribute less than 5%.

Page 26906, lines 17-21: It isn't clear exactly what these results mean. Is it saying that 38+13%=51% of the total AOD variability in SE Asia is captured by the two modes within the dotted spatial regions? Or by the spatial regions relative to the whole domain? Or by the two modes relative to all modes within the dotted regions? This needs to be rewritten.

Page 26906, lines 24-25: What exactly is meant by "86% correlation"? Does it mean that the correlation coefficient is 0.86? Or the coefficient of determination is 0.86? Is it saying that this principal component controls 86% of the variability in AOD? Observed or predicted? And over the whole domain, or the dotted boxes? This is also quite unclear. The whole of Section 3 suffers from a lack of clarity over exactly what is being discussed. 'Correlation' is also used without clarity of meaning elsewhere, e.g. P26910.

Page 26907, lines 24-25: Cloud cover has long been known to be a limit to aerosol remote sensing in this region: this paragraph presents this as if it is new information, only realised by the authors in the present study and the prior study Cohen (2014).

Figure 5-7: It looks like the model is quite poor at predicting AERONET AOD: timing is reasonable but the range of variability is in general a lot larger in the real data than predicted by the model. This again raises the question of what the purpose and utility of this model are. The main conclusion seems to be that you can use fire counts and decreases in vegetation cover to predict the occurrence of smoke, which is basically the methodology adopted for many biomass burning emissions inventories (i.e. not an unsurprising or new result).

I have not provided comments on the rest of Section 3 and onwards because I feel that the above are enough to justify rejection of the manuscript on the grounds of lack of novelty/utility, and lack of clarity of the discussion.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 26895, 2015.